A RAND NOTE

The Base of Sand Problem: A White Paper on the State of Military Combat Modeling

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RAND

PREFACE

This Note in the form of a White Paper improves upon a draft circulated in early 1990 to senior Department of Defense (DoD) officials and military officers. The original draft came about because the authors had separately concluded that the DoD's approach in developing and using combat models, including simulations and war games, is fatally flawed—so flawed that it cannot be corrected with anything less than structural changes in management and concept.

This white paper was originally written in March, 1990. We updated it in mid-February, 1991, but did not attempt to reflect either changes in the world situation or new modeling and simulation initiatives in DARPA and OSD.

The draft built upon discussions at a small ad hoc workshop convened at RAND for several days in December, 1989. The working group consisted of senior scientists and analysts from RAND, the Lawrence Livermore National Laboratory, and the Jet Propulsion Laboratory. All present had conceived, built, and used military models, including sophisticated war gaming systems. Their experience extended from developing and validating physics-level algorithms to analyzing alternative theater and global military strategies and arms control agreements. Further, participants had worked at the technological frontiers of simulation, including knowledge-based simulation. They had had many successes and had learned from partial failures. The participants, then, were not gadflies, professional or organizational critics of the DoD, academic purists, or anti-model Luddites. Rather, they were insiders of the national security model-development and analysis communities. They were attempting to sensitize the DoD to a serious national problem and the need for drastic remedies. The draft in 1990 appears to have contributed to that goal, based on the numerous unsolicited responses we received. We hope this final version will be useful to a broader community.

This Note was sponsored by the Defense Advanced Research Projects Agency (DARPA). It was developed in RAND's National Defense Research Institute (NDRI), a federally funded research and development center sponsored by the Office of the Secretary of Defense and the Joint Chiefs of Staff. The original workshop and draft were accomplished using NDRI's research support funds. Comments are welcome and should be addressed to Dr. Paul K. Davis, RAND's Corporate Research Manager for Defense Planning and Analysis. The Internet address for electronic mail is Paul_Davis@rand.org. Mr. Blumenthal is a consultant to the Lawrence Livermore National Laboratory.

SUMMARY

The DoD is becoming critically dependent on combat models (including simulations and war games)—even more dependent than in the past. There is considerable activity to improve model interoperability and capabilities for distributed war gaming. In contrast to this interest in model-related *technology*, there has been far too little interest in the *substance* of the models and the validity of the lessons learned from using them. In our view, the DoD does not appreciate that in many cases the models are built on a base of sand. Nor does it appreciate that while replacing the sand with a more nearly solid foundation is feasible, it will be extremely difficult in scientific, intellectual, and managerial terms.

Fig. S.1 describes the base of sand as chaos in the modeling community. Many models and tools are simply inadequate. Further, they are seldom verified or evaluated well, there is confusion about what the models assume and do, and there have been many wasted efforts and lost opportunities. The principal contributors to this chaos, which are themselves problems, are (moving clockwise from the right in Fig. S.1): (a) inadequate theories, methods, standards, and practices for modeling and evaluation; (b) related computer hardware and software problems, such as achieving interoperability and software quality; (c) dissonance and lack of discussion across communities and organizations (e.g., operations planners vs defense planners); and, importantly, (d) the lack of a vigorous military science.

Upon looking into each of these contributors in more detail, we conclude that *an overarching problem is a variant of "No one is in charge."* There currently exists no national office with the responsibility of encouraging, nurturing, and sponsoring activities necessary to relieve the chaos described in Fig. S.1.

In considering remedies, it is important to recognize the need to invigorate the study of military science—shifting the balance between art and science farther to the science end of the spectrum, but doing so with full recognition that military science is a social science, beset with uncertainties and variables that cannot accurately be measured or anticipated. Such a science requires theories and associated models, coupled with activities to test and inform them (Fig. S.2). We recommend explicitly distinguishing between two overlapping categories of models: *research models*, which collectively embody and communicate our knowledge (including alternative theories and both objective and subjective data), and *application models,* which can be seen as tools allowing us to solve particular problems or inform decisions. By and large, policymakers and analysts have conceived models as mere ad hoc tools, to the detriment of continuity and coherence of military science. At the same time,

those building more comprehensive models have typically not appreciated their limitations for analysis (they are often too complex for policy analysis), nor designed them as research models. The DoD needs to understand the different roles of research and application models and should sponsor and nurture both within a framework of an evolving military science.

For both research and applications, combat models should be viewed less as answer machines than as frameworks for summarizing and communicating objective and subjective knowledge (including knowledge of uncertainties), and as mechanisms for *exploration*. This view, which is especially important in designing complex research models, establishes stringent requirements for model transparency, comprehensibility, and flexibility. Increasingly, it is also important that models be designed so that they or their modules can be directly compared with, used in, or used with other models. This is crucial for scientific reproducibility and peer review, for efforts to calibrate aggregated models using higher resolution models, for analysis of empirical data, and for distributed war gaming. The theories and methods for designing models with these purposes in mind are not currently well understood.

In this paper, we recommend that the Secretary of Defense establish an Office of Military Science (OMS) to plan and administer the process of creating the national environment necessary for a vigorous military science. The OMS would not conduct research itself, but would instead encourage, nurture, and to some extent sponsor it—although relying primarily on the Services and other agencies for the vast majority of research and analysis. The OMS would be concerned primarily, but not exclusively, with research and research models rather than direct applications.

The OMS would (1) sponsor conferences, journals, and the development of textbooks; (2) sponsor development and iteration of experimental technical standards and data dictionaries to make model interoperability and model comparisons feasible and attractive; (3) encourage, coordinate, and sometimes sponsor historical and other empirical research to inform model building and evaluation; (4) disseminate optional standard model modules and data bases (to be thought of as baselines rather than something "blessed"); (5) sponsor cross-cutting military-science research that would not otherwise be accomplished, including model comparisons and "countermodeling" efforts; (6) support development of methods for the use of models and decision aids in distributed and conventional war gaming; and (7) facilitate the exchange of both research and application models and data bases.

We do not emphasize a czar-like OMS role for verification, validation, or accreditation, because verification and validation (V&V) are difficult to define and must often be accomplished in the context of a particular study. Further, because of the uncertainties

ACKNOWLEDGMENTS

The authors appreciate workshop discussions with Dr. Richard Hillestad and Dr. John Friel of RAND, Dr. Ralph Toms of Livermore National Laboratory, and Dr. Joseph Feary and Dr. Hugh Henry of the Jet Propulsion Laboratory. RAND colleague Bruce Goeller provided a detailed and thoughtful review. Many other people also provided comments and suggestions. Finally, we wish to acknowledge the many colleagues in the analytic community with whom we have talked about the issues in this Note over the last few years—colleagues in FFRDCs, national laboratories, civilian and military agencies, and commercial companies. We hope that our critical assessments will be recognized as being directed at processes and mindsets, not individuals or organizations, and that none of the criticisms we provide will in any way diminish the many and substantial advances in modeling that have occurred in the last decade. There are no villains in the story we tell here.

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FIGURES

I. INTRODUCTION TO THE PROBLEM

PRELIMINARY COMMENTS ON TERMINOLOGY

This Note refers frequently to "models," "simulations," and "war games." Unfortunately, these terms have multiple and contradictory meanings in common English. For example, "models" include the ship in a bottle, theories of expansion of the universe, and political scientists' conceptual frameworks. "Simulations" include the playing through of procedures by a team of workers, various types of models and computer programs, and electric-train networks enjoyed by hobbyists. There are some relatively general definitions, but none of them can encompass all the usages, because the usages are contradictory. In this paper, therefore, we adopt a particular set of definitions as follows (not because our definitions are better than others, but because they will simplify discussion here):[1]

a. A *model* is a mathematical or otherwise logically rigorous representation of a system or a system's behavior. It may or may not be computerized. It may or may not be structured as a game. It may or may not attempt to represent the internal functioning of the real system. It may be abstract only, or it may be implemented as a computer program, a nomogram, pencil-and-paper procedures, or in a variety of other ways. For brevity, we will also refer to the implementation (e.g., a computer program) as a model, even though the distinction (e.g., program vs model) is sometimes important.

b. A *simulation* is a special kind of model that represents at least some key internal elements of a system and describes how those elements interact over time. Most combat simulations are implemented as computer programs. The principal exceptions are manual war games, discussed below. Computer simulations may be *closed*, in which case the user "pushes the button" and the computer generates a complete simulation. They may instead be optionally *interruptible*, in which case the user may intervene during the simulation and change assumptions in midstream. *Interactive* simulations may be optionally interruptible or may instead demand that users provide information and decisions during the simulation. Some simulations (closed, interruptible, and interactive) are structured as games—i.e., there may be explicit model entities representing opponents and allies.

[1]See Hughes (1984), Rothenberg (1989), and Anderson, Cushman, Gropman, and Roske (1989) for careful discussions and model taxonomies. Other definitions have been proposed by "the Gorman Panel" in its work to establish a draft charter for a DoD oversight office for modeling and simulation.

THE INCREASINGLY CRITICAL ROLE OF MODELS

Factors at Work

We assume that readers already recognize generally the importance of models and that there are long-standing problems of verification and validation. They may be less aware, however, that DoD dependence on combat models, and particularly on simulations, is increasing rapidly. There are several factors at work:

- Scenarios, weapon systems, operational concepts, and forces are changing *qualitatively*.
- There is no experience base to guide much of the related planning, although the war with Iraq is mitigating this problem in some respects.
- Computer simulations, including war games, will be replacing many field exercises.
- Distributed war gaming in particular will be a major, and perhaps the principal, mechanism for joint and combined theater-level coordination and training of commanders.
- Computerized decision aids are becoming increasingly critical in command-control processes.

If these represent demand factors for modeling, then supply factors matter also; the supply changes taking place in computer science, information systems, simulation, and distributed war gaming are revolutionary, not incremental.

Scenarios, Forces, and Concepts

To elaborate on these themes, consider first that the changes taking place in Europe and the Soviet Union are transforming the strategic landscape and that the DoD's future planning cases will be highly diverse with respect to location, allies and antagonists, scale of combat, nature of combat (e.g., maneuver vs positional warfare), and many other factors. The old standby scenarios of Central Region defense at the intra-German border and Southwest Asian defense against the Soviet invasion of Iran are obsolete. They will be replaced by a broad range of scenarios that will include defense of the unified Germany, defense of post-war Kuwait and Saudi Arabia, contingency actions, such as assisting Poland in crisis (deterring Soviet reentry into Eastern Europe), and other specific and generic affecting battle outcomes.[3] Further, many assessments will involve combat phenomena not arising in the war with Iraq (e.g., operations when we do *not* have absolute air supremacy), and these will be driven by models and very limited field tests.

This increased dependence on models (including, remember, war games) has already been occurring with the emergence of SIMNET at the military-technical level and as distributed war gaming has become a reality under the leadership of NATO's Supreme Allied Commander, General Galvin. The technological opportunities now exist to use these tools far better than before. The results could be in some ways far superior to those achieved with traditional exercises, which are procedural, scripted, narrow in scope, and complicated by masses of people and activities that obscure key issues. At the same time, there is really no choice but to use models more extensively, because economic and political limitations will greatly constrain the number, scope, and realism of field exercises in the future.

Decision aids are also becoming increasingly critical. Already, commanders and senior staff depend heavily on computerized information displays from the weapon-system level up. Most of these displays reflect implicit models, since it is models that dictate what information one wants to see. At the weapon-system level this is often well understood, but decision aids going beyond relatively straightforward presentation of data and oriented specifically to higher-level field commanders are still relatively primitive (e.g., aids to help evaluate different courses of action or to find courses of action to achieve specified objectives). The same is true of models for assessing alternative defense programs.

Technological Opportunities

We are now well into the information era, and increasingly it is recognized that information dominance is becoming critical in warfare. Fortunately for the United States, we have advantages in the most relevant technologies, which include not only communications and computing power, but also model-related technology, such as new programming languages; analyst workstations; relational data bases; man-machine simulation systems, such as SIMNET; and knowledge-based modeling concepts. Unfortunately, however, there is a problem that has already become a limiting factor in what can be accomplished, one that is not yet widely recognized. We call this the base of sand.

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[3]To illustrate how critical the use of combat models is in analyzing empirical data, consider that battle outcomes have historically borne no relationship to the raw force ratio. By contrast, when the outcome data is passed through models sensitive to situational factors such as terrain, preparations, asymmetries in fighting effectiveness due to better organization and training, and so forth, one finds that the data actually makes sense and that what matters is a ratio of *effective* forces. Unfortunately, the values of some of the key variables may not be known in advance. As a result, the models are sometimes more useful for after-the-fact description than for reliable prediction.

Attitudes

- *Minimal Empiricism.* Little money is spent collecting empirical information to inform higher-level models. Much is spent in developing orders of battle, but little on improving the underlying assumptions about combat or other operational processes. In particular, there is much too little *systematic* effort to collect, structure, or exploit historical data.[8]
- *Parochialism and Ignorance.* There is little discussion and analysis across levels of resolution, across different perspectives of conflict, or even across models allegedly describing much the same thing.[9] Instead, there are distinct ingrown communities, each with its own biases.[10]
- *Dubious Acceptance Criteria.* The criteria for model acceptance by military organizations often have little to do with empirical or historical information. Instead, they have to do with not disrupting operations (e.g., the new model should get the same answers as the old one, but faster), representing the organization's particular interests (e.g., the high potential effectiveness of a particular weapon or force), agreeing with the subjective impressions of the most senior relevant officer, and being compatible with existing computer data bases. In such an environment, the search for "truth" may not even be considered relevant.[11]

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on our first draft even mentioned kindly some of our own work, as well as their own. We agree that there are *many* successes for the analytic community to be proud of, but we believe our overview depiction here is not overstated.

[8]An exception here over the years has been the work of Trevor Dupuy, who with small and sporadic funding over the years has been the leader in using historical information to inform highly aggregated combat modeling. The Army's Concepts Analysis Agency has also supported useful historical work and continues to do so (e.g., McQuie, 1988, and Fain, et al., 1988; see especially Helmbold, 1990, for abstracts and a bibliography). However, these have been largely independent efforts without much guidance from or input to simulation modelers. There is usually a mismatch between the form of the historical work's output and the information needed by simulation modelers and analysts. For example, Dupuy's and McQuie's work organizes historical information in a way most suitable to *static* models. Nonetheless, it has been exceptionally valuable. For other all-too-rare examples of valuable empirical research, see Hughes (1986), Rowland (1986), and Molnar and Colyer (1988). These deal, respectively, with naval tactics, the effects of suppression, and the effects of interdiction.

[9]The principal exceptions occur *within* organizations. For example, both U.S. Air Force Studies and Analysis and the FRG's Industrielagen-Betriebsgesellschaft GmbH (IABG) have used hierarchies of models with some success, and within RAND and the Institute for Defense Analyses there are some recent examples of work using together models of greatly different character and resolution (e.g., JANUS and both corps- and theater-level models). The Army's Model Improvement Program has attempted such work across Army agencies. Doing such things well, however, is difficult, time-consuming, expensive, and socially complex. Further, the *theory* for doing so is lacking.

[10]By contrast, in the traditional scientific and engineering disciplines, it is recognized that one must be able to move from one level of resolution to another. For example, statistical mechanics is used to explain and enhance the laws of thermodynamics, but the laws and measurements of thermodynamics are the peg points that the more detailed theories must agree with to be valid.

[11]By contrast with that of the U.S., the Soviet military regards it as a sacred duty to pay attention to historical information—a failure to do so may mean the lives of many soldiers in a future war.

maneuver combat in the new Central Europe or elsewhere. The rules of thumb underlying the aggregated models are invalid, and the current detailed models are not typically useful for policy analysis. Many of the detailed models also assume stereotyped tactics, limiting their value for maneuver studies.

The Modeling and Analysis Process

- *Failure to Converge.* Studies often reach contradictory conclusions without addressing those contradictions and without the sponsors insisting that they do so—especially if the sponsors are in different organizations. Although we (the authors) like to believe that we, our organizations, and numerous colleagues in the analytic community do somewhat better in this regard, we believe nonetheless that the search for and convergence on truth is not a strong, much less dominating, ethic in the analytic community or most of the sponsoring offices. [14]
- *Pro-Detail Bias.* Most funding organizations favor detail in models, whether or not the detail is justified for the studies anticipated. For example, weapon-on-weapon calculations are often preferred over force-on-force calculations. The effect is to proliferate uncertain parameters. The parameter values, although sometimes systematically generated from more detailed models that are organizationally "accepted," often produce results that are demonstrably incredible (e.g., predicted attrition rates far higher than has ever been observed historically). Thus, the pro-detail bias complicates and obfuscates, but does not necessarily improve analysis. It *might,* if there were sufficient effort to establish a scientifically valid base of parameter values under a distribution of battlefield conditions and stochastic factors, but that is not what typically occurs.[15]
- *The Myopia of Aggregated Analysis.* At the other extreme, methods and models using force scores (e.g., WEI/WUVs, ADEs, EDs, or DEFs) are sometimes applied precisely where they are most inappropriate—in making cost-effectiveness comparisons of alternative divisional structures or total-force structures. Light infantry divisions always fare poorly in such work—even though in more detailed analysis accounting for realities of terrain, they can appear highly cost-effective for

[14]One observer not in an FFRDC has commented: "Many individuals are engaged in combat modeling, but little cross-fertilization occurs. Peer review, independent verification and refereed publication do not exist. In general, the professional environment is one of beauty contests and political competition, closely held knowledge, and a fear of exposition. Because all simulation systems in use contain serious flaws, and because of the fierce competition for turf, money, and the ear of senior officers and officials, to expose one's work for professional examination and critique is to commit suicide."

[15]For an unflattering but perceptive parable on the Services' approach to models and analysis, see Builder (1989, pp. 107ff).

all of the models are structurally biased—omitting known but "soft" influences of air forces, although in different ways. More detailed physics-level simulations are probably needed, as well as new historical analyses to better characterize—by situation—the observed effects of tactical air forces on maneuver and logistics.[18] We also need specialized field tests and innovative types of gaming to provide at least bounding information. To our knowledge, at least, there has been little systematic and "scientific" cross-model, cross-discipline effort to illuminate the issue, although recent work on close-support led by Bruce Don and Fred Frostic at RAND suggests that much can be done with modern simulation and visualization techniques; also, recent parametric work on the Persian Gulf demonstrates how critical countermaneuver and counterlogistics effects probably are (Shlapak and Davis, forthcoming). The war with Iraq may greatly improve our empirical base.

As a final example of how the analytic community has failed policymakers and general officers planning operations, consider that in November, 1990, when the United States was contemplating the need for *offensive* ground operations against Iraqi forces in Kuwait, the combat models being used throughout most of the analytic community focused almost exclusively on *defensive* operations by the United States and its allies. Neither the models nor many analysts were prepared to explain what would be required for a successful allied offense (e.g., to investigate in detail how U.S. tactical air forces and B-52 strategic bombers could be best employed to support an offensive, taking into account qualitative Iraqi weaknesses).[19]

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[18]One interesting compilation of historical information on this is Molnar and Colyer (1988), done at the Warrior Preparation Center.

[19]Presumably, the Joint Chiefs of Staff and the Central Command were exceptions here, conducting realistic human war games and analysis in support of deliberations about the offensive option, but we can only speculate on the matter.

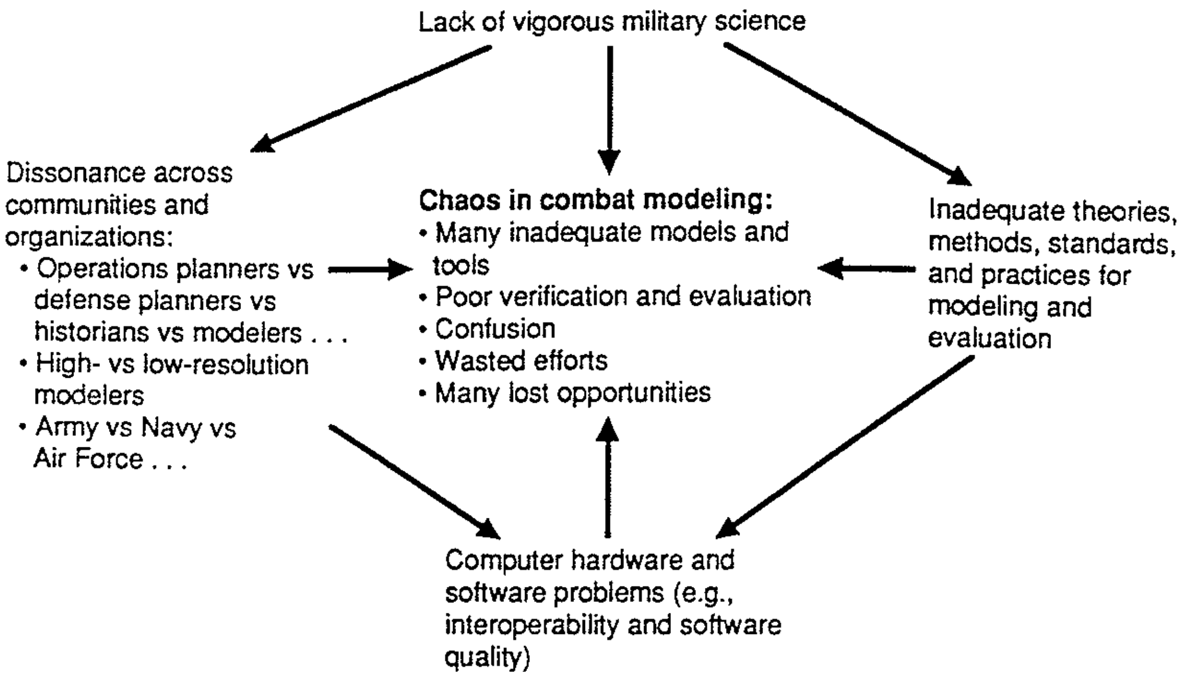

Fig. 2.1—Factors influencing chaos in combat modeling

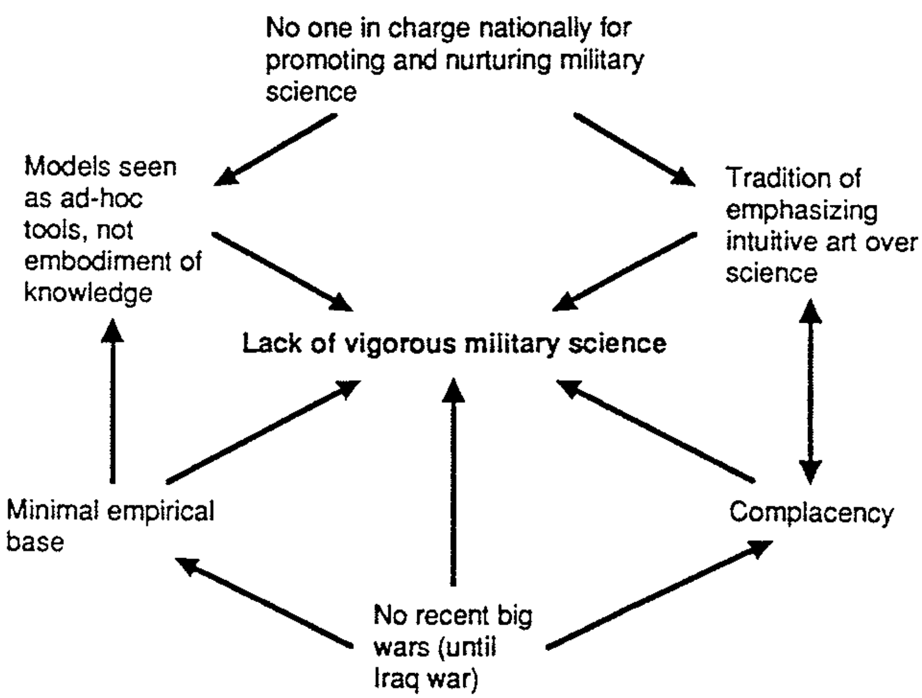

Fig. 2.2—Factors influencing the lack of military science

models. Other organizations have done similarly. The atmosphere, then, is better than in previous years, because people are looking afresh at problems.

DISSONANCE ACROSS COMMUNITIES AND ORGANIZATIONS

Another factor contributing to chaos is, according to Fig. 2.1, the dissonance across important boundaries.[7] For example:

- Operational commanders often have difficulty communicating with combat modelers and civilian analysts concerned with building and explaining the defense program, and vice versa.
- Combat modelers working with high-resolution models, such as JANUS or VIC, typically interact seldom and poorly with modelers and analysts working at lower resolution (e.g., analysts seeking to characterize military balances, assess alternative operational strategies, or assess the potential value of different force structures). It is easier to do better when the individuals are part of the same organization, but even then, the difficulties are substantial.
- Modelers and analysts in or working for the various military services often have difficulty communicating well, because they see the issues so differently, and each is in an organization that is competing for scarce resources.

Fig. 2.3 indicates some of the influences that cause this dissonance. The lack of military science is primary, because it is from a science that one draws theories and methods to accomplish cross-cutting work. Again we cite the role of complacency and the absence of a national authority to encourage, nurture (and, frankly, enforce) cooperation. Parochialism and competition are major factors here, although the Goldwater-Nichols act and the resulting reorganization may be influencing the situation favorably by strengthening the coherence of the Joint Staff and the roles of the Chairman of the Joint Chiefs of Staff and CINCs. There has been considerable evidence of high-quality joint and combined planning in the war with Iraq and, before that, in the invasion of Panama. Similarly, there has been increased attention to such matters in the war-college curricula and studies conducted within the DoD and in some of the FFRDCs.

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[7] This has troubled one of us for some years and influenced design of the RAND Strategy Assessment System (RSAS) (see Davis, 1986).

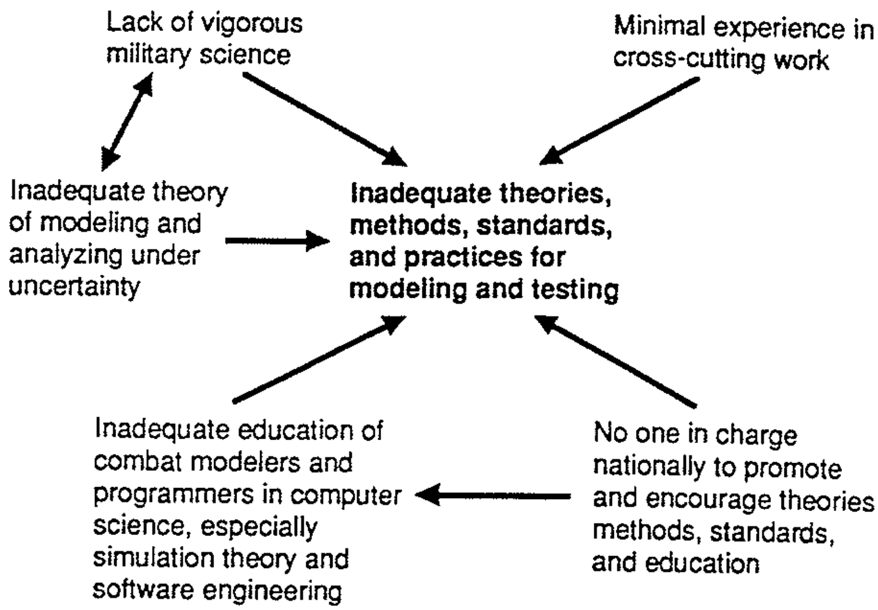

Fig. 2.4—Factors influencing inadequacy of theories, methods, standards, and practices for modeling and testing

across levels of resolution or across submodels built by different organizations. Various organizations, such as the Warrior Preparation Center, Air Force Studies and Analysis, RAND, and IDA, have at least some experience with such matters, but we do not believe any of them would claim a high degree of rigor. To the contrary, the lashups have typically been accomplished with the software equivalent of bailing wire. Germany's IABG seems to have done better over the years.

As with the other problem areas identified in Fig. 1.1, an overarching problem here is that no one is in charge (Fig. 2.4, bottom right)—in this case, to promote and encourage the needed theories, methods, standards, education, and so on.

COMPUTER HARDWARE AND SOFTWARE PROBLEMS

The remaining major contributor to chaos is, according to Fig. 2.1, various computer-related problems such as the noninteroperability of models, computers, and graphics programs, and the uneven quality of model software (i.e., the software programs implementing the models).[9] Fig. 2.5 identifies what we see as major factors in this problem

[9]Here we distinguish between the quality of models and implementing software programs, having in mind a distinction between substantive content and such software characteristics as structure, performance, and maintainability. As noted earlier, however, well-designed models provide the specifications for good model software.

software to mature and reach potential), and the lack of both mechanisms and incentives for verification, evaluation, and straightforward comparisons.

- In addition, the quality problem is due significantly to the lack of a well-developed theory of simulation modeling *for complex systems*. Ultimately, model software is limited by the quality of the model specifications, and these are often developed by people with no formal training in modeling and, often, little education in advanced mathematics or science.[12]
- As noted in the influential book, *The Mythical Man Month*, top-notch programmers are a factor of ten more productive than less capable ones (Brooks, 1982, p. 30). Qualitatively, at least, the same is true for modelers and analysts. The moral here is that one cannot successfully dc serious combat modeling on the cheap.

Having provided a system-level diagnosis of the problem, let us now discuss in somewhat more detail the issue of military science. Does it already exist? Can it exist? Should it exist?

THE ISSUE OF MILITARY SCIENCE

Conflicting Counterassertions

Our assertion that the United States (and West) lacks a military science (or at least a vigorous one) is sometimes challenged for both of two opposite reasons. Some claim that there *is* a military science: It is what professional military officers learn over decades of experience and the study of history, doctrine, and problem-solving procedures worked out in countless field units and headquarters. Others claim, by contrast, that there can be no real "military science," because the phenomena at issue are too complex and too muddied by human factors, such as fear, genius, and fatigue, and by random factors, such as weather. In this view, one should talk of the "art" of war, as did Sun Tzu. Let us consider each view briefly.

Is There Really No Military Science? Art vs Science

Is it valid to claim that there is no military science in the United States, or at least no vigorous one? We believe the answer is "yes," although there have been important informal

[12]Another problem here is that simulation modeling is not a large, well-developeu, or prestigious discipline. Few texts and few departments specialize in it, especially in simulation modeling of truly complex systems. Jay Forrester's work in Systems Dynamics at MIT was an exception. So also is the work of Bernard Zeigler at Arizona State (see, e.g., Zeigler, 1984 and 1990). RAND's RSAS represents a major effort to model military complexity from a top-down perspective (Davis and Hall, 1988).

military campaigns, as recommended by Napoleon). The knowledge may not extend explicitly to how one should deal with unusual situations and why.

The enduring *Principles of War* are an example of the military art. They are fuzzily stated and to some extent mutually contradictory. They are unquestionably of fundamental importance, but the way they are stated and the fact that there is relatively little structured guidance on how to resolve the contradictions is part of what we mean by there being no military science. As another illustration of military art, consider how much of the "real military knowledge" is embodied only in organizational practices (e.g., at the level of an Air Force wing) rather than in textbooks, formal doctrine manuals, or journals laying out the various problems and situations methodically.

Finally, we assert that anyone attempting to learn about military operations so that he can build a good combat model needs no convincing that the U.S. military approaches the subject of war as an art rather than a science: He can't find anything remotely like the textbooks he used to learn other disciplines. His view is reinforced when he sees how little effort goes into empirical studies or attempts to understand and reproduce the work of others as part of learning and converging on "truth."

Can There Be a Science Amidst the Friction of War?

The other view is that there can be no real military science, because of the manifold uncertainties in war. Proponents of this line sometimes wax eloquent on the subject, and usually quote Clausewitz (although, in our view, distorting his message). Quoting from a recent article warning against the folly of slipping into a body-count mentality, which in turn is quoting Clausewitz:

> They [those who overquantify] aim at fixed values, but in war everything is uncertain, and calculations have to be made with variable quantities. They direct the inquiry exclusively toward physical qualities, whereas all military action is intertwined with psychological forces and effects. They consider only unilateral action, whereas war consists of a continuous interaction of opposites. Military activity is never directed against material force alone. It is always aimed simultaneously at the moral forces which give it life.[15]

One *might* conclude from these comments of Von Clausewitz that numbers and calculations are irrelevant and that one should fall back on intuition and military genius. However, such a conclusion would be a mere excuse for sloppy thinking. Furthermore, concluding from such arguments as those quoted that there can be no military science reflects a serious misconception of science. Science (and engineering) abounds with problems

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[15]Harry Summers, "Body Count Proved to Be a False Prophet," *Los Angeles Times*, February 9, 1991.

that embody all one's knowledge in a particular subject (including knowledge about alternative theories and hypotheses and various types of objective and subjective data) and building a model to help inform decisions on a particular question, such as what weapon system to buy, what level of forces to maintain, what operational strategy to employ, or what doctrine to adopt. The reality is that when models are used for decision support, they must be relatively simple, by which in practical terms we mean they must be comprehensible to those seeking to use them. This implies that models for decision support must have fewer variables, fewer relationships, and fewer processes than models attempting to represent "what's going on" in detail.[18] These application models also embody knowledge, but more selectively—so much so that in some cases they are ad hoc one-time constructs tailored specifically to the question at hand.

Research Models

What form should we expect our research models to take if they are to be a repository and embodiment of knowledge? The answer, of course, is that we need a variety of different model types. In some instances, one is interested in characterizing relationships, steady-state conditions, endpoints, or optimal strategies. There are roles in such cases for such diverse types of models as static models, closed-form analytic models (e.g., solutions of Lanchester equations), game-theoretic optimization models, and artificial intelligence models using heuristics to find good strategies.

This said, however, we argue that since combat is inherently a dynamic process involving the interaction of many entities, and since a considerable part of military science deals with understanding those interactions over time, it follows that simulation models—including rather complex computer simulations—are especially important.[19] There seldom is a good alternative if one wants to capture and understand dynamic cause-effect relationships in complex systems. Unfortunately, current large models are not usually designed with enough attention on transparency and comprehensibility for them to fulfill the role of representing and communicating knowledge adequately. That can be changed with time,

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[18]To illustrate what we mean here, a recent RAND study dealing with post-war requirements for defense in Kuwait and Saudi Arabia depended primarily on a parametric analysis using a highly aggregated simulation model implemented as a spreadsheet program on the Macintosh computer. This proved very useful in discussions with both civilian officials and officers in CENTCOM and the Joint Staff—discussions focused on identifying key variables and key issues rather than precise answers. The work was complemented, however, and to a significant extent calibrated with, more detailed simulations and war games using the RSAS (See Shlapak and Davis, forthcoming).

[19]Another implication here is that the modeling must seek structural validity in the sense of representing correctly, at whatever level of aggregation, the systems' natural entities and cause-effect relationships. It is not enough to find statistical relationships from historical data. Indeed, it is better to use physical insight and experience-based judgments about cause-effect relationships than to omit what are believed to be the key variables of the problem.

result, there are many "squishy" aspects and the need for forthright use of subjective judgments as well as quantitative data.

"equivalent divisions" (EDs) vs time. Such analysis can cut through the morass of complexity and focus attention on critical issues (e.g., the importance of prompt response to strategic warning, strategic mobility systems, and allies). Such analysis can be embellished for specific problems and made to represent rather more complex issues (e.g., the difference in anticipated operational-level force ratios in different corps sectors, perhaps to explain the importance of achieving the time goals of a POMCUS program for Europe or Saudi Arabia).

In contrast, other strategic- or theater-level analysis may employ closed theater-level simulation and gaming with hundreds of entities and an explicit representation of terrain. So, for example, the SACEUR or the Commander of AFCENT would not be satisfied with war planning or analysis expressed in the simplified terms of the policy analysis. Such commanders should, however, be interested in *other* simplified treatments abstracting the principal elements of specific problems on which they are working. For example, theater commanders are often comfortable with the concept of "equivalent divisions," however flawed the calculation of equivalent division scores may be in detail. They are also comfortable—for *some* purposes—with simplified depictions of concentration, such as that shown in Fig. 3.2, which represents *one* perspective of a situation that also involves, for example, terrain, lines of communications, and force quality, none of which are shown. For other purposes, those same commanders need detailed depictions of one or more aspects of the problem. The rule of thumb here is that a commander at one level will sometimes need to see depictions of the situation that are two to three levels more highly resolved than his natural level. For example, a corps commander will sometimes need to see the deployments and missions of individual battalions (which are determined by brigade commanders subordinate to division commanders, who are subordinate to the corps commander).

The points to be made here are:

- There is a fundamental need for *variable resolution models* (or families of models) in which there is true consistency across levels[2] and for concepts and methods making it easier to do cross-resolution work, including work with models not originally designed to be compatible.[3]

[2]Such consistency is often difficult to achieve, but what we are talking about is the same consistency one has in going from lower to higher scale maps within a family. As those familiar with theoretical work relating microscopic and macroscopic worlds in physics recognize, one cannot expect to produce accurate aggregated depictions by merely integrating high-resolution depictions; to the contrary, because of the propagation of uncertainties in high-resolution "data," the results of such integration are typically inaccurate (e.g., implausibly high attrition rates). Consequently, a sound theory must allow for inserting empirical and judgmental data at several levels in the overall system, including at the top level (e.g., with historical division-level attrition rates), and then iterating to achieve a multilevel stability and consistency.

[3]RAND has begun studying such issues in detail under DARPA sponsorship.

We emphasize these matters because chronic misunderstandings exist about issues of resolution. Individuals and organizations working with high-resolution models commonly look down upon aggregated models and aggregated rules of thumb (e.g., the "3 to 1 rule"), and those working with low-resolution policy-oriented models similarly look down upon the more detailed models. It is uncommon for modelers and analysts to move back and forth among levels of resolution and perspectives.[4] In years past, doing so was not feasible computationally, but the limiting factors now are conceptual and organizational.

A COMPREHENSIVE APPROACH

In addition to considering alternative levels of resolution and perspective in the modeling itself, a comprehensive approach needs to attend seriously to all the items in Fig. 3.3, which highlights the fact that at least three different classes of activity can add to our body of data: experiments (including exercises), historical analysis, and systematic interviewing of officers with relevant experience. Data, in turn, should inform theory, especially in the form of research models of various types. Theory leads to simplifications and specializations in the form of application models, analytic methodologies, and tools such as graphical aids. The theories, methodologies, and tools all contribute to strategy,

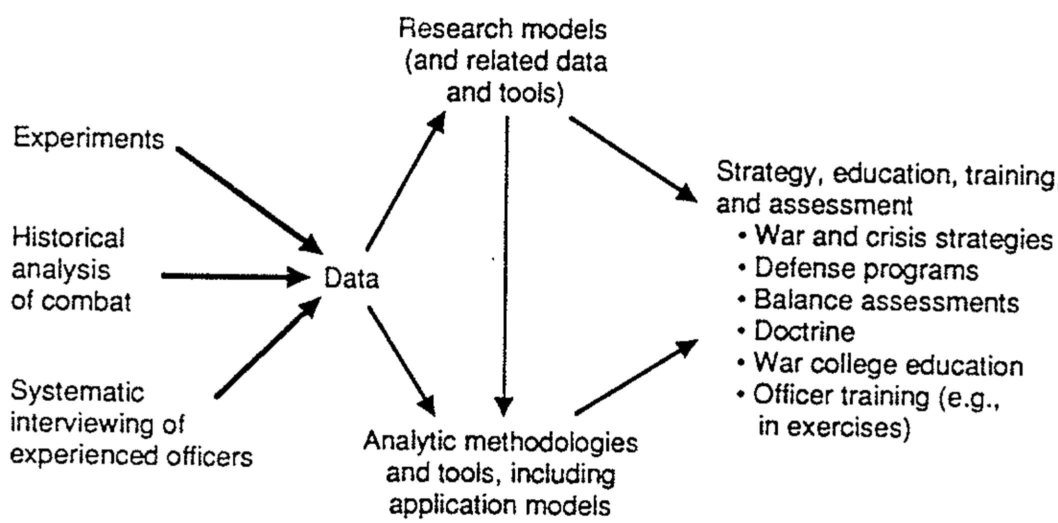

Fig. 3.3—Components of a comprehensive approach

[4]One reason that the participation of general officers is important in the development of sound and useful models and methods is that they have worked at different levels and from different perspectives and are better able to appreciate the need for all of them. Whereas junior staff almost invariably seek more detail (in areas in which they are knowledgeable), general officers and some senior staff often appreciate or seek abstraction.

- Models (or families of models) should be developed hierarchically to correspond to real-world command levels.
- Different *kinds* of models should exist to serve different needs, but there should be strong professional and governmental pressures to establish relationships among them (e.g., comparisons, calibrations, or merely clear descriptions of similarities and differences), as well as to validate them on their own terms, which may be quite different from class to class of models.[7]
- Overall modeling activity should not be dominated by any one approach (i.e., neither the "bottom up" nor the "top down" approaches are "right").
- Models should support multiscenario analysis in many dimensions (e.g., Davis, 1988a). This requires unusual flexibility and so many degrees of freedom as to make those seeking cut-and-dried answers uncomfortable.
- Variable-resolution features should be designed in from the outset, as should features allowing for iterative cross-level calibration, tuning, and consistency checking. As noted earlier, calibrations should use low-resolution data, as well as high-resolution data. The bottom-up paradigm is wrong.
- Models should reflect stochastic effects, at least optionally. Further, deterministic operations should be based on results of stochastic modeling. This may require distinguishing among cases rather than settling on expected-value calculations, because outcome distributions are so often skewed or multimodal.
- Models should be sensitive to command-control choices at all levels; they should include optional submodels optimizing or satisficing decisions as a function of information and criteria; they should also include optional submodels describing *likely* (and probably very nonoptimal) decision processes, accounting for human limitations well known in behavioral psychology (e.g., the inability to understand "sunk-cost" issues). Decision models should operate on the basis of perceived information, not perfect information, except for limiting-case analysis.
- Adaptive strategies and tactics should be emphasized; scripted strategies and tactics should be avoided, since they teach stereotypes and encourage complacency.
- Models should be sensitive to Reconnaissance, Surveillance, and Target Acquisition (RSTA) factors.

[7]Some models are calibrated roughly to historical data; others use test-range weapon data as inputs, although they usually include scaling factors to bring their results into rough accord with expectations. Neither approach is "correct."

THE ENVIRONMENT

If military simulation is to reach its full potential, it should be conducted in an intellectual environment with the following features:

The Research Environment

The first requirement is to have the concept of research and development (R&D) be as strong with respect to combat modeling as it is in other aspects of DoD work. Such R&D funding needs to be steady and focused on the midterm and long term. We cannot overemphasize how difficult a problem area this is.[12] The funding and management environment should also include

- Long-term funding of military combat modeling as applied science, not mere tool building.
- Substantial redundancy and overlap of research—precisely the opposite of what is frequently recommended by panels—but with strong incentives and requirements for open publishing and comparison efforts.
- Protection of research from such common problems as organizational shortsightedness (e.g., an emphasis on doing the current job faster rather than better), corruption (e.g., an emphasis on supporting the organization's natural positions in the adversary process), and parochialism (e.g., ignoring the roles of political factors, allied military forces, other services, or other theaters).
- Exposure to sunlight through peer-review processes and publications.
- Standards and practices to improve model verification and validation (by which, in practice, we may mean evaluation), and to make model transfer and interoperability feasible.[13]
- The development of academic-quality textbooks.

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[12]As data points, models such as JANUS, JESS, and the RSAS are developed over periods of many years and cost tens of millions of dollars, ultimately. Success requires dependence on top-quality senior people. Our development experience also redemonstrated the principles cited in Brooks' *The Mythical Man Month* in that a large fraction of the work was accomplished by a small number of key people with exceptional talent. Such trailblazing efforts can no more be accomplished by people of average talent or education than analogously important efforts can in engineering or physics.

[13]We emphasize again, however, that complex combat models cannot be "validated" in the sense of certifying their correctness: There are simply too many uncertainties and unknowns. Further, a centralized effort to "certify" only certain models and data bases could be *seriously* counterproductive. "Blessed models" are not the same as "valid" models, and a great deal of mischief can be accomplished under the rubric of centralized quality control and certification.

(FFRDCs) and national laboratories have a long track record as being attractive places to work.

It is worth noting here that defense work will probably be much less attractive for graduating students in the future than in the recent past as a result of the Cold War ending. The war with Iraq will mitigate this, but only temporarily, even if the war goes well. Any actions that enhance the *technical and scientific* content of defense work will tend to counter these trends and permit a continuing supply of talent.

relatively more concerned with research, research models, and relatively generic methodologies and tools than with specific applications, although it should also be interested in *assisting* agencies with their near-term problems, which would buy good will and establish important contacts for information and insights.

In assessing effectiveness of models for describing phenomenology or supporting classes of analysis, the OMS should consider political-military factors. Further, it should strongly encourage research and analysis emphasizing issues of operational strategy, tactics, and command-control, not just weapon-system effectiveness. It might also sponsor cutting-edge work on decision aids for commanders and staff at all levels, but here as elsewhere it should insist on relevant empirical studies and not be satisfied with theories and flashy computer systems. Most application models, including decision support systems, should be developed by user agencies, but "research models" should include models experimenting with decision support.

In all of its work the OMS should propagate the spirit of scientific inquiry, including the associated requirements for documentation, free exchange of ideas, peer review, and development of theories and texts. It should, for example, sponsor and otherwise encourage both "comparative modeling" and "countermodeling" activities, such as have proven very useful in other domains of policy-relevant simulation (see, e.g., Greenberger, et al., 1976).

In thinking how to implement the concepts introduced here, we recognize that a variety of options are plausible. We believe, however, that a new organization is desirable because of the need to change deeply seated attitudes. We suggest, as the starting point for discussion, creating an OMS that would report to the Secretary of Defense through a Military Science Board (MSB) consisting of

- A three-star general officer appointed by the Chairman of the Joint Chiefs of Staff
- An assistant-director-level representative of the Director of DARPA
- A DASD-level representative of the ASD (PA&E)
- A comparably ranked representative of the Director, Defense Research and Engineering
- A comparably ranked representative of the Assistant Secretary for Force Management and Personnel (specifically, from an office charged with training responsibilities)
- Senior representatives from the Army, Air Force, Marines, and Navy.

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the certification process to gain power over other government organizations and to limit competition with outside organizations.

ENLISTING THE FFRDCs AND NATIONAL LABORATORIES

The DoD already has institutions that are well suited for this type of research, some of the FFRDCs and national laboratories.[3] Further, some of them already conduct a great deal of related research. None of them, however, has anything so broad and systematic on their research agendas. Further, there is little cross talk among these institutions, which has reduced the quality and impact of their work from what might have been possible. We recommend that the DoD initiate immediately a limited program of cooperative research involving several of these institutions. RAND and Livermore have shown interest in the recent past and we believe there would be interest from some of the others as well.[4]

ESTABLISHING LINKS WITH THE WARRIOR PREPARATION CENTER AND JOINT WARFARE CENTER

We have been impressed by the Warrior Preparation Center's (WPC's) ability to work with commanders and respond to their requests in short periods of time. It is likely that the WPC and the Joint Warfare Center (JWC) will demonstrate a high degree of competence, innovation, and a "can do" spirit; it is also likely they will continue to have warm ties to operational and training commands. We therefore believe it is desirable to bring them into the research program suggested in this White Paper by giving them a charter for early testing of prototype models and decision aids.[5]

AN AGENDA FOR IMMEDIATE AND NEAR-TERM ACTIONS

With this background, then, we suggest the following actions:

- DARPA or the Secretary of Defense should sponsor a study to define a detailed management plan and research agenda for an OMS. This could involve a Blue-Ribbon committee, a research project, or both. There should be participants from outside the defense community (e.g., academic figures interested in defense and knowledgeable about the real-world mechanisms of scientific inquiry, especially in fields with inadequate empirical data). The research agenda should address all

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[3] In addition, of course, the DoD has in-house offices and agencies, such as Air Force Studies and Analysis and the Army's Concepts Analysis Agency. These do some original research and regular contract research with commercial contractors in addition to their extensive analysis functions. The DoD has consistently concluded over the years, however, that it was essential to go outside the government for a large part of its advanced research and some of its analysis.

[4] We also recommend cooperative studies with non-U.S. military analysis groups in the FRG, UK, and elsewhere.

[5] This idea has been proposed and discussed at a number of workshops in the last several years, starting with the DARPA-sponsored workshop at RAND on distributed war gaming in 1988 (Bankes, forthcoming). At the same time, we believe that neither the WPC nor the JWC are appropriate places for basing scientific research, even of the applied variety of interest here, because the organizations' natural dispositions are to support here-and-now needs, cutting corners as necessary.

science. This might be stimulated by a joint memo from, for example, the Director of DARPA, the Director of Defense Research and Engineering, the Director of OSD's Net Assessment, and the Director of J-7 or J-8.

- Long-lead-time plans should be made to fence budgets for FY 1991 and thereafter. A funding stream might be $10M in FY 1992 and a constant level thereafter at $30M in 1991 dollars. A substantially larger budget might be necessary, depending on the scope and charter of the OMS that we postulate.

CONCLUSION

In this white paper, then, we have sought to sensitize policymakers and senior military officers to the need for a true military science within the United States and to suggest both substantive and procedural action items. We have done this without the benefit of a detailed study, drawing instead on our personal experience and the ideas that we could pull together quickly in discussion with our colleagues. Our principal recommendation is that the DoD should recognize that it is not enough to build models, or even to build and "manage" them. Nor is it feasible to achieve good results by merely imposing a military-style discipline on the undisciplined community of modelers and analysts. Nor is it feasible to "validate" models by creating a program to do so.[8] Instead, the DoD should nurture development of a vigorous military science. Accepting this as a prime objective will shape fundamentally the DoD's approach to the building, testing, comparing, and using of models in all their varieties from closed-form analytic models through computerized simulations and war games. Currently (February, 1991), the DoD seems likely to pursue an effort to provide oversight on modeling and simulation, but it has only begun to discuss seriously the issue of military science. We hope this white paper will contribute to that discussion.

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[8]As discussed by Clayton Thomas in his chapter in Hughes (1984), verification and validation are very hard even to define, much less accomplish. In our view, decisions about model acceptability will have to be made by the organizations using them, and not by higher level centralized organizations working without the benefit of context.

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